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Mapping European amphibians and reptiles: collective inquiry and scientific methodology

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ABBREVIATIONS

AFIE: Association Française des Ingénieurs Ecologues.
ASIH: American Society of Ichthyologists and Herpetologists.
CITES: Convention on International Trade in Endangered Species of Wild Fauna and Flora.
IEGB: Institut d'Ecologie et de Gestion de la Biodiversité.
IUCN: International Union for the Conservation of Nature.
L-E: *Rana lessonae-esculenta* population system.
MNHN: Muséum National d'Histoire Naturelle, Paris, France.
NMW: Naturhistorisches Museum, Wien, Austria.
P-G: *Rana perezi-grafi* population system.
R-E: *Rana ridibunda-esculenta* population system.
SEH: Societas Europaea Herpetologica.
SFF: Secrétariat de la Faune et de la Flore.
SHF: Société Herpétologique de France.
SPN: Service du Patrimoine Naturel.
SSAR: Society for the Study of Amphibians and Reptiles.
UTM: Universal Transverse of Mercator mapping system.

INTRODUCTION

In 1983, shortly after its founding, the Societas Europaea Herpetologica (SEH) established a Mapping Committee, which was entrusted with the charge of preparing a distribution atlas of amphibians and reptiles in Europe. Under the leadership of Jean-Pierre GASC (Paris), and with the technical support of a service of the Paris Museum (MNHN) first known as "Secrétariat de la Faune et de la Flore" (SFF) and renamed in 1995 "Service du Patrimoine Naturel" (SPN), this Committee worked for 15 years

and produced the *Atlas of amphibians and reptiles in Europe*, which was published in July 1997. This 500-page volume is the result of a truly international work which involved several hundred persons all over Europe for the collection of original field data, and about a hundred authors for the writing of the texts devoted to the species. Altogether, this *Atlas* is based on 85,067 "species/square/period" data concerning 3940/4362 (i e. 90 %) of all the 50 × 50 km UTM squares covering Europe (within the limits defined by MERTENS & WERMUTH, 1960). Of these data, 41,704 (i.e. 49 %) concern the 62 amphibian species and 43,363 (i.e. 51 %) the 123 reptile species recognized as valid in this work.

The first part of the volume contains a general presentation of the methodology used to prepare this *Atlas*, and overviews of the European climate and vegetation, of the paleogeography of the European herpetofauna, and of problems posed by the conservation of this fauna. This is followed by analytical data concerning the species: each species account consists of a map of reported occurrences (except for five marine turtle species), and of basic information, comments and bibliographic references. Although each account is signed by one or several author(s), it is clear that only the written part of the account is to be credited to the latter, while the maps are the result of the collective inquiry (actually, each map bears a mention of copyright by "MNHN/SPN & SEH"), and should therefore be quoted as being by GASC et al. (1997). These accounts are followed by three appendices, i.e. updatings of the lists of European amphibians and reptiles, and a table presenting the official conservation statuses and levels of threats of European herpetological species. Finally, a bibliography of about 2500 references and an index to taxa are provided.

The book, of format 21 × 29 7 cm, is soft-covered, which may be appropriate for a volume to be read once or twice, but less so for a book of frequent use. It is well printed, with nice maps in white, grey and blue. Printing mistakes are rather numerous, and suggest that reading of the final proofs has been too quick (e.g , just in the introductory chapters and the appendices: p. 4, "french"; p. 6, "Europran"; p. 11, 13: "Oural"; p. 11, "In particular, was necessary"; p. 15, "data was", p. 16, "many various", p. 21, italics, p. 29-30, several misprints; p. 31, one paragraph repeated, "Portugese"; p. 31-32: "systematic", "speciesi"; p. 405, "collectind", "beguen"; p. 406, "hybridisation"; p. 407, "to determinate"; p. 412, "occuring"). Unfortunately, the book was edited and printed in France, and the editors did not properly care for the way words should be hyphenated at the right margin of lines, so that they were so according to the French rules, not the English ones (see e.g., WOOLF, 1974; SUMMERS, 1995; PROCTER, 1995): thus, in English, the proper division is "men-tioned", not "mentio-ned" (p. 8), "standard-ised", not "standar-dised" (p. 9), "chal-lenge", not "challen-ge" (p. 9), "famil-iar", not "fami-liar" (p. 9), "pro-duce", not "produ-ce" (p. 10), etc. Black and white drawings of some species are provided, but on p. 4 of the book the authors of part of them only are acknowledged. e.g., the nice drawing of *Discoglossus pictus* of p. 494 is not credited to its proper author, namely Jean-Jacques MORÈRE (this authorship had already been ignored in the original publication of this drawing, on the cover of *Bull Soc. herp. France*, **5**, January 1978).

Interestingly, according to this *Atlas*, there is a significant difference between the distribution of species richness of the two studied zoological groups all over Europe. while the combined map for all amphibian observations (p. 34) clearly points to a larger species diversity in central Europe, the combined map for all reptiles (p. 160) not less clearly shows a higher diversity in southern Europe, mostly in the Mediterranean region Unfortunately, this finding is not discussed at all, in particular in the light of the following question: does this difference reflect a genuine biological fact, which would then call for a scientific explanation (climatic-ecological, historical, or both), or does it simply reflect a different distribution of observers of both groups all over Europe?

The Mapping Committee of SEH is to be commended on having been able to carry this collective undertaking to its term. Altogether, the amount of work which has been necessary for the production of this book is impressive, as are the wide geographical and political scope of the inquiry, covering about 40 different countries from the Atlantic to the Ural and to the Caucasus, and the high number of collaborators involved in it No doubt this book will become a major reference for a number of European governmental and official bodies, who need basic documentation about the distribution and conservation status of native animal species in order to be able to take administrative and legal decisions concerning their management, collecting, transport, commerce and protection The genuine interest of such official bodies for this kind of works is emphasized by the fact that this *Atlas* was largely supported financially by the French Ministry of the Environment, who had already funded the production and publication of the two editions of the French distribution atlas of amphibians and reptiles (CASTANET, 1978, CASTANET & GUYÉTANT, 1990)

But the potential interest and impact of such a work is much wider In the *Preface* of the European *Atlas* (p. 9), Wolfgang BÖHME rightfully writes: "The careful documentation of distribution data is the most important prerequisite for evaluating the situation of animal species in a given geographical frame. This frame (...) provides invaluable zoo-geographical information, from both historical and ecological points of view, on the taxon concerned. This helps us understand the history, and estimate the future, of animal populations." In other words, reliable chorological data are answers to "what" questions (see MAYR, 1997) that provide information irreplaceable for answering the "how" and "why" questions that phylogenetic or ecological research ask, and for being able to properly deal with the threats that many European herpetological species are currently facing. According to the geographical area covered and to the scale chosen, distribution maps can provide different kinds of information. On the scale of a region, and especially if the latter represents a significant geographical unit for the organisms studied, such maps can help to better understand the ecological requirements of species, phenomena of competition, altitudinal limits and some conservation problems. On a national scale, maps can contribute to determining the responsibilities of states regarding their natural heritage. Finally, on a continental scale, maps can provide an interesting light on the biogeographies of species, or even of genera or families. Needless to say, in all these cases, to be able to play correctly their role, distribution maps must be produced with all the care and rigour usually required for scientific works. Are these conditions met with in this European *Atlas*? I will consider this question under several points of view. As this review is written for readers of a batrachological journal, I will concentrate here mostly on examples taken in the amphibians, but most of the statements below are also valid for the reptile sections of the volume.

TAXONOMY AND NOMENCLATURE

In the short anonymous text entitled "How to use the Atlas" (p. 31-32), one can read: "The nomenclature used in the atlas is the one prevailing at the time the texts were written Where there is no consensus, the author of the text is responsible for choosing the nomenclature used. The scientific name may designate a species complex, according to current knowledge (1997)." (p. 31). However, in the methodological introduction (p. 11-16), H. MAURIN, P. HAFFNER, H. DA COSTA & J.-F. BRULARD provide a slightly different information, since they state that the 185 species recognized as valid in the *Atlas* were so "according to the nomenclature as it stood in 1995" (p. 11), and that subsequent changes could not be taken into account: "because time was very short, it was not possible to process the newly described or newly found species within the standard procedure. There was no time to collect the data necessary for the distribution maps or to find authors willing to write the accompanying species' reviews." (p. 13) Some comments regarding these changes were therefore added in some of the species accounts and in the special appendices "updating the lists of species", by A. OHLER and I. INEICH, that appear at the end of the book (p. 404-407).

Even if the imprecise "1995" landmark is to be understood as "1st January 1995", it was not always respected in the book, and still more so if it is "31 December 1995". Some species described or recognized as valid well before the beginning of 1995 were not duly considered in the body of the *Atlas*, and are only listed in the appendices (p. 404-407). Among amphibians, the most striking case is *Rana pyrenaica*, whose original description (SERRA-COBO, 1993) was published in March 1993, and included a detailed distribution map of the known localities of the species, which could well have been integrated in the *Atlas* after transcription into UTM squares. Other amphibian examples include *Triturus carnifex*, *Triturus dobrogicus* and *Triturus karelinii* (BUCCI-INNOCENTI et al., 1983, MACGREGOR et al., 1990), *Rana cerigensis* and *Rana cretensis* (BEERLI et al., 1994), and a few other taxa (*Salamandra corsica*, *Bufo verrucosissimus*, *Hyla sarda*, *Rana bergeri*), which have recently been considered valid species by some authors, although published evidence for such taxonomic decisions is lacking (see DUBOIS & OHLER, 1995*a* and OHLER's appendix to the *Atlas*) Similar problems exist in reptiles, not all of which were mentioned in the appendix: to give just one example, the oriental populations long referred to *Hemidactylus turcicus* are now referred to other taxa (see e.g DELAUGERRE & CHEYLAN, 1992: 57), which is ignored in the contribution by U. GRUBER (p. 211).

The *International Code of Zoological Nomenclature* (ANONYMOUS, 1985; quoted below as "the *Code*") was not always understood or respected by contributors to the book, so that the latter contains a

number of nomenclatural mistakes. In amphibians, several of them concern the green frogs of the subgenus *Rana (Pelophylax)*, whose nomenclature was reviewed by DUBOIS & OHLER (1995*a-b*) and CROCHET et al. (1995), a fact which is only briefly mentioned in the appendix of the *Atlas* (p. 404-405) but should rather have been taken into account in the body of the book itself: thus, the species reported in the book as *Rana balcanica*, if valid (see BEERLI, 1994; BEERLI et al., 1996), should be known as *Rana kurtmuelleri*. The book does not include any discussion or reference concerning the nomenclatural problems which have recently been raised regarding the genus of Plethodontidae successively known as *Geotriton*, *Hydromantes* and *Speleomantes*. In several works, B. LANZA argued that the European and American salamanders of this group should be placed in two distinct genera (LANZA & VANNI, 1981, LANZA et al., 1996). In these works as well as in the *Atlas*, this author decided to use the name *Hydromantes* for the European species of this genus, apparently because he considered that the International Commission on Zoological Nomenclature would "almost certainly" take the decision which had his preference (see LANZA et al., 1996: 17, 21). But this prediction proved wrong, as the Commission decided that the European species, if considered generically distinct, should bear the name *Speleomantes* (ANONYMOUS, 1997). Possibly the parts of the *Atlas* concerning this genus were written before publication of the Commission's Opinion in March 1997, but then the text should have been corrected before publication, in order to follow this decision which has force of law for all zoologists worldwide, irrespective of their personal tastes. Even before the Commission had voted on this case, the authors of the *Atlas* should have used the name *Speleomantes* for the European species, since, as had been shown by SALVIDIO (1995) and DUBOIS (1995*b*), no general "current usage" could be claimed to exist in this case, as two parallel usages were in force after the publication of DUBOIS's (1984*a*) paper: while most North American authors continued to use the name *Hydromantes* for salamanders of this group, a clear tendency developed in Europe, including in several "official lists", to replace it by the name *Speleomantes*. This mistake, and even worse, the fact that the *Atlas* does not discuss this case at all, is unfortunate, as this volume will become an important international reference and will contribute to the spreading of an incorrect nomenclature and to the continuation of a regrettable situation of nomenclatural confusion. An example of this confusion is to be found in the *Atlas* itself: in J. P. MARTÍNEZ RICA's contribution on climate and vegetation, both names *Hydromantes* and *Speleomantes* are used as valid names in different paragraphs of p. 21!

Some mistakes are also to be found in the *Atlas* regarding the valid spelling of scientific names, the nomenclatural availability of scientific names, the author's names and dates of nominal taxa, or the inclusion of these names and dates in parentheses. The name "Chamaeleontidae" is properly written in pages 6 and 201, but misspelt "Chamaeleonidae" in pages 26-27 (J.-C. RAGE), the name "Trionychidae" is misspelt "Tryonichidae" in page 27 (J.-C. RAGE), and the subspecific name "*Triturus alpestris bukkiensis*" is misspelt "*bükkiensis*" in pages 72 (A. ZUIDERWIJK) and 492 (index). The name "*Molge syriacus* Valenciennes, 1877", listed by L. J. BORKIN in p. 86 among the synonyms of *Triturus vittatus*, has no status in nomenclature: it was first published by LATASTE (1877: 365) as a synonym of *Triton vittatus*, and was not adopted as a valid name before 1961, so that by virtue of Art. 11.e of the *Code* it is not an available name, and has no therefore no type-specimen, contrary to the statement of THIREAU (1986: 74-76). Several cases of incorrect authorship and date can be pointed out in amphibians: the nominal species *Alytes muletensis* was created by SANCHÍZ & ADROVER (1979), not by "SANCHÍZ & ALCOVER (1977)" (no publication corresponds to this reference), as written by J. P. MARTÍNEZ RICA in p. 92, the nominal species *Rana balcanica* by SCHNEIDER & SINSCH (1992), not by SCHNEIDER, SINSCH & SOFINANIDOU (1993), as stated by T. S. SOFIANIDOU in p. 130 (see DUBOIS & OHLER, 1995*a*: 179-180); and the nominal species *Rana dalmatina* by FITZINGER in BONAPARTE (1838), not by BONAPARTE (1840), as written, after many others, by K. GROSSENBACHER in p. 134 (see DUBOIS, 1984*b*: 117-118). In reptiles, according to Art. 50 of the *Code*, the author of the nominal subspecies *Podarcis hispanica cebennensis* is FRETEY (1986: 81), not "GUILLAUME & GENIEZ in FRETEY (1986)", as stated by C. P. GUILLAUME in p. 278; the mention by GUILLAUME & GENIEZ (1986) of a specimen figured in FRETEY (1986) as the "holotype" of this subspecies results in the designation of a lectotype for the latter (Art. 74.b) and in a restriction of the type-locality to Valros (Hérault). The *Code*'s principle of coordination requires that the nominative subspecies of *Agkistrodon halys* bear the same author and date as the species, which is ignored in the *Atlas* by I. S. DAREVSKY (p. 378). The authors's names and dates of *Bufo bufo verrucosissimus*, *Eryx jaculus turcicus*, *Eryx miliaris miliaris*, *Natrix tessellata heinrothi* and *Macrovipera lebetina obtusa* (if the latter genus is recognized as valid) should be enclosed in parentheses, as these nominal species-group taxa were created

in other genera, which is ignored in the accounts dealing with these taxa. Finally the *Atlas* contains several cases of confusion between two terms of the *Code* having distinct meanings and uses, i.e. "nominotypical" and "nominal" (see ANONYMOUS, 1985): in amphibians, the latter is used for the former in the contributions on *Triturus helveticus* (A. ZUIDERWIJK, p. 78), *Triturus marmoratus* (A. ZUIDERWIJK, p. 82) and *Bufo viridis* (P. ROTH, p. 122), while the correct term "nominotypical" is used under *Triturus* superspecies *cristatus* (J. W. ARNTZEN & L. BORKIN, p. 76) and *Bufo bufo* (L. J. BORKIN & M. VEITH, p. 118).

Although for reptiles most recent generic taxonomic changes were duly considered, for amphibians the generic taxonomy used in the *Atlas* is the "traditional" one, found e.g. in the checklist of MERTENS & WERMUTH (1960). Subgenera are not recognized, not even discussed. However, several taxonomists have proposed or adopted a subgeneric classification for some genera of European amphibians and reptiles, including the amphibian genera *Hydromantes* (see discussion above), *Triturus*, *Alytes*, *Bombina* and *Rana*. In two cases at least (genera *Triturus* and *Rana*), presentation of the species under their respective subgenera would have been useful: mixing all European Ranidae in alphabetical order under the generic name *Rana* is much less enlightening for the reader than would have been their separate listing under the subgeneric names *Aquarana*, *Pelophylax* and *Rana* (see DUBOIS, 1998*a*).

Several other examples clearly stress the little concern of the authors and editors of the *Atlas* for taxonomic and nomenclatural matters. The first one is that of the so-called partial "synonymies" which are provided at the head of each species account, under the heading "Main synonyms". These are stated to include "the synonyms most frequently used in literature" (p. 31). According to the *Code* (ANONYMOUS, 1985: 266), a synonym is "each of two or more scientific names of the same rank used to denote the same taxon". In this definition, a "scientific name" is to be understood as a Latin name validly published under the *Code* to designate a new taxon. Posteriorly to its original publication, such a name is liable to be *modified*, in its spelling, combination or onymorph (see SMITH & PEREZ-HIGAREDA, 1986), but this does not result in the creation of a *new name*: such modified names are not synonyms of the original name, but merely different "name-forms" of the latter, which have no independent status in nomenclature. They should therefore not appear in a *synonymy* sensu stricto. They may appear in a *synonymy and chresonymy* (see SMITH & SMITH, 1973) or *chreso-synonymy*, either *complete* or *partial*, but the difference between the two should be clearly understood and mentioned (see e.g. DUBOIS, 1997*b*: 184-185). Complete confusion exists in the *Atlas* regarding the status of the partial "synonymies" provided: over 62 amphibian species recognized as valid in the book, only 27 % of the "synonymies" provided deserve the qualification of genuine synonymies, while the other 73 % are partial chreso-synonymies (31 %), mere partial synonymies (34 %) or wrong synonymies (8 %). Here are the details for each of these categories:

(1) Genuine synonymies (total 17). (a) Complete synonymies (including cases where no synonymy is provided and no synonym is known to exist) (total 11): *Chioglossa lusitanica*, *Salamandra atra*, *Salamandra lanzai*, *Alytes cisternasii*, *Pelobates syriacus*, *Hyla meridionalis*, *Rana epeirotica*, *Rana iberica*, *Rana latastei*, *Rana macrocnemis* and *Rana perezi*. (b) Partial synonymies (total 6): *Bufo bufo*, *Bufo viridis*, *Rana catesbeiana*, *Rana dalmatina*, *Rana ridibunda* and *Rana temporaria*.

(2) Partial chreso-synonymies (total 19): all species of the genera *Salamandrella* (1), *Pleurodeles* (1) and *Salamandrina* (1); *Euproctus platycephalus*, *Salamandra salamandra*, *Triturus boscai*, *Triturus helveticus*, *Triturus vittatus*, *Triturus vulgaris*, *Alytes obstetricans*, *Bombina bombina*, *Discoglossus sardus*, *Pelobates cultripes*, *Pelobates fuscus*, *Bufo calamita*, *Hyla arborea*, *Rana arvalis*, *Rana* kl. *esculenta* and *Rana lessonae*.

(3) Mere partial chresonymies (total 21): all species of the genera *Speleomantes* (as "*Hydromantes*") (6), *Mertensiella* (1) and *Pelodytes* (2); *Euproctus asper*, *Euproctus montanus*, *Triturus alpestris*, *Triturus cristatus*, *Triturus italicus*, *Triturus marmoratus*, *Triturus montandoni*, *Alytes muletensis*, *Discoglossus montalentii*, "*Rana balcanica*", *Rana italica* and *Rana shqiperica*.

(4) Wrong synonymies (total 5). (a) Names of valid subspecies listed as synonyms (total 3): *Bombina variegata*, *Discoglossus galganoi* and *Discoglossus pictus*. (b) No synonymy provided, although synonyms are known to exist (see e.g. MERTENS & WERMUTH, 1960) (total 2): *Proteus anguinus* and *Rana graeca*.

The account for *Proteus anguinus* by J. DURAND starts with the following statement (p. 50): "Main synonyms: None". Then, a few lines below, one can read: "Fitzinger (1850) described 7 species of *Proteus* ( ). These species are today invalidated; nevertheless Mertens & Wermuth (1960) still mention 12 different names. We may consider there is one species and possibly 2 to 3 subspecies (...)". Such statements

can be understood under the pen of authors of species accounts who are not taxonomists, but it would clearly have been the responsibility of the editors of the *Atlas* to care for the quality and homogeneity of the information. As this has clearly not been done, users of this book should be warned not to rely on these "synonymies", but to rather use other works providing serious synonymies (e.g., for European amphibians: MERTENS & WERMUTH, 1960; DUBOIS, 1995*a*; DUBOIS & OHLER, 1995*a-b*, 1997*b*).

A similar warning of caution can be made for another section that appears in all species accounts, under the heading "Terra typica". Beside the fact that this designation is not that recognized by the *Code*, which uses the formula "type locality", two major problems appear regarding this section. First, the type-locality is provided only for the nominal taxon whose name is currently the valid one of the taxonomic taxon. However, this information has only nomenclatural, not biological, value and interest, and, if given for the valid name, should also be provided for its synonyms (for more details, see DUBOIS, 1987*b*: 104-107). Second, the authors of the *Atlas* have taken for granted so-called "restrictions of type-localities" which were not accompanied by lectotype or neotype designations, although such restrictions are clearly invalid under the *Code* (see DUBOIS & OHLER, 1995*a*: 146, 1997*a*: 312-313, MYERS & BÖHME, 1996: 17-18). As long as no such type designations have been made, such invalid restrictions may be "provisionally retained", and, if this proves possible, for the sake of stability it may be justified to "validate them a posteriori" through lectotype or neotype designation (DUBOIS & OHLER, 1997*a*: 313). But this is not always possible or desirable and, at any rate, as soon as a valid *objective* restriction of type-locality through lectotype or neotype designation has occurred, neither the "original type-locality" nor subsequent invalid restrictions are in force any more. Thus, the designation by DUBOIS & OHLER (1997*b*: 334) of a figured specimen as lectotype of *Rana arborea* Linnaeus, 1758 restricted the type-locality of this nominal species to the region of Zürich (Switzerland), and the original type-locality of LINNAEUS (1758: 213) ("sub foliis arborum Europae, Americae") is not valid any more, contrary to the statement of A. STUMPEL on p. 124 of the *Atlas*; similarly, the type-locality of *Rana* kl. *esculenta* Linnaeus, 1758 is Nürnberg (Germany) through the designation by DUBOIS & OHLER (1995*a*: 149) of a figured specimen as lectotype, not through the two successive so-called "restrictions" by MERTENS & MULLER (1928: 19; 1940: 18), still recognized as valid by R. GÜNTHER on p. 138 of the *Atlas* (despite his citing DUBOIS & OHLER, 1995*a*). According to the *Code* (Art. 72.h), the type-locality of a nominal species-group taxon is the "place of capture or collection" of its name-bearing type, not any other locality where it may possibly have come from, except in the case of unnatural transportation by man: therefore, the type-locality of *Proteus anguinus* is the Cerkniško jezero (lake Cerknica) in Slovenia south of Ljubljana (see HABIC, 1993), the place where had been collected the single specimen (holotype) on which LAURENTI's diagnosis (1768. 37) and figure (1768: pl. 4 fig. 3) were based, and the subsequent so-called "emendation" of this locality by FEJERVARY (1926) is invalid, as is the multiple type-locality given for this species by J. DURAND in p. 50 of the *Atlas*.

"Common names" in several languages are provided for all species considered valid in the *Atlas*. In fact, such names are not "common", "current" or "vernacular" names at all, as most of them were coined specially for a recent book (STUMPEL-RIENKS, 1992) and have not yet been significantly used in the respective countries where these languages are spoken. For the time being, and until they are widely used in popular literature, they should rather be regarded as proposals, not as "official" names. Then, some other proposals, some of which (DUBOIS, 1982*b-c*; MATZ & WEBER, 1983) are anterior to STUMPEL-RIENKS's (1992) and some others (DUBOIS & OHLER, 1995*a*) cover species not considered in the latter work and with a different rationale for selection of names, should also have been mentioned in the *Atlas*.

The last paragraph of taxonomic relevance provided for each species concerns their "European subspecies". This paragraph also is quite unsatisfactory for any reader interested in taxonomic and evolutionary problems. Why were only "European" subspecies mentioned for all species? Europe is a political, not a natural zoogeographical unit, and mention of data concerning extra-European range and subspecies of the "European" herpetological species would be very useful in such a book. No homogeneity exists in this volume concerning the information provided for the "European" subspecies. The authors and dates of the subspecific names are given in most cases, but not always. In amphibians, this information is wanting for *Triturus alpestris* (A. ZUIDERWIJK, p. 72), *Triturus vulgaris* (S. L. KUZMIN & A. ZUIDERWIJK, p. 88), *Discoglossus pictus* (M. VEITH & H. MARTENS, p. 104) and *Bufo viridis* (P. ROTH, p. 122). Type-localities and areas of distribution of the different subspecies are given for a few species only. This is all the more strange as the concept of subspecies, at least as used by modern taxonomists (e.g., MAYR & ASHLOCK, 1991), is equivalent to the older concept of "geographical race", and is eminently

"mappable": if well defined, the subspecies of a given species have different, allopatric or parapatric, distribution areas, that can easily be shown on a map. On the maps of the *Atlas*, it would have been useful and enlightening to use different symbols to show the occurrence of different subspecies and of possible intermediate populations or hybrid zones, or to add, e.g. as broken lines, the known or supposed limits of the subspecies areas, and contact or hybridization zones between them. This simple mapping would have helped pointing to the existence of taxonomic problems concerning the validity of some of the currently accepted subspecies: if mapping of subspecies limits appears difficult, it may well be an indication that the subspecies are poorly defined and need revision. For example, it would be most enlightening to map the so-called subspecies of *Bombina variegata*, with their type-localities and supposed ranges (see ARNTZEN, 1978), in order to see what comes out concerning the so-called "subspecies" *scabra* and *kolombatovici*, which are supposed distinct but whose type-localities are very close. Finally, beside bringing information about geographic variation, it would have been particularly important to pay more attention to subspecies in this volume for two major reasons: (1) for conservation problems (see below the *Triturus alpestris* case); (2) because many taxa currently regarded as subspecies are likely to be considered species in the future (see DUBOIS, 1998*a*): the existence of maps for the subspecies would then have been readily available to future authors as a first evaluation of the range of these species.

The importance of taxonomic and nomenclatural problems pointed out above in the "well-known" animal group of European amphibians may appear strange to some readers. The fact is that the taxonomy of this group, like those of reptiles and of many other animal and plant groups on our planet, is still far from being "finished and stabilized", and that a lot of work, and of novelties, can still be expected in this field (for more details, see DUBOIS, 1998*a*). At any rate, to be valid and useful, any chorological work must be based on a reliable and up-to-date taxonomy and nomenclature, and on a good knowledge of the taxa to be mapped (see DUBOIS, 1998*b*). In the absence of a serious, professional, taxonomic basement, any zoogeographical work is bound to encounter other kinds of problems, which will directly affect the validity of the zoogeographical data themselves, as we will now see.

CHOROLOGICAL DATA

Even a cursory survey of the *Atlas* immediately shows that the distribution maps presented are of various quality, accuracy and completeness. Some, especially those of species with a limited distribution, were apparently prepared on the basis of excellent field, literature and/or collection data and seem quite reliable. But this is not the case of all maps. I will concentrate here on a few examples taken in Ranidae, but unfortunately these are not the only ones, and the methodological problems raised by these examples are important enough to throw a shadow of doubt over the entire book, as a reliable source for chorological data on the European herpetofauna

The first example is that of European green frogs of the subgenus *Rana (Pelophylax)*. For sure, the evolutionary status and taxonomy of these frogs is a complex one, which has only recently started to be disentangled (GÜNTHER, 1979; GRAF & POLLS PELAZ, 1989; OGIELSKA et al., 1995) However, if a distribution atlas is to be of some genuine scientific help and significance, it is precisely in such difficult cases! The least that can be expected from such a book in such complex situations is to point out problems and difficulties, rather than "erasing" them under seemingly accurate maps based on wrong data and contributing in fact to spread confusion and misunderstandings. It is clear that, for the time being, identification of live specimens of European green frogs is difficult, if not impossible in the field without having recourse to bioacoustics or to laboratory techniques such as protein electrophoresis or morphometrics, and use of all these methods requires quite specialized knowledge and experience. For this reason, distribution data on these species based on written answers to questionnaires should be accepted only with considerable caution. In most cases, and even when the information came from well-known and serious observers, the only possible serious use of such data is to regard them as mere evidence of presence in the surveyed region of "green frogs" of the subgenus *Rana (Pelophylax)*, without further precision. In many cases, electrophoretic or morphometric study of specimens has revealed the presence in some regions of forms or species of green frogs unsuspected in these areas. This problem has become particularly serious because, especially since the development of deep-freezing food techniques, green

frogs are more and more used as a source of human food in Europe, which has resulted in wide-scale commerce and transportation of these frogs, and also of the American species *Rana (Aquarana) catesbeiana* the problems of *genetic pollution* (DUBOIS & MORÈRE, 1979, 1980; DUBOIS, 1983*d*, 1990), and of *faunistic pollution* (DUBOIS, 1983*c*), that this new commercial development has caused, were analysed in detail by DUBOIS (1976, 1977, 1983*c*, 1985*a*). Without reference to the previous works, these problems were recently "rediscovered" by REINERT (1991) and ARANO et al. (1995). This large-scale displacement of frogs contributes to obscure the patterns of distribution of green frogs in Europe and explains that, for zoologists interested in the intriguing problems posed by the evolution of this exceptional complex of species and kleptons, it would seem imperative to oppose all projects of commercial exploitation of the populations of these frogs, which do not constitute a traditional or first-priority food for most Europeans. Unfortunately this attitude has not been shared by all individuals and associations concerned with herpetological matters (see DUBOIS, 1983*c*, 1985*a*), so that these problems will undoubtedly take a growing importance in the forthcoming years, and that the "original" distribution of green frogs in Europe, before their transportation by humans, will probably never be possible to trace, at least in all details.

Authors of a scientific distribution atlas must be aware of these problems (1) of identification of specimens and (2) of genetic and faunistic pollution, and should at least mention them in the discussions of such a work. Unfortunately, except in one case (see below), such discussions are badly wanting in the European *Atlas*. Beautiful distribution maps of green frogs in Europe are provided, but without the necessary information or warning concerning these problems. Let us consider some of these maps.

First, let us compare the maps provided for *Rana* kl. *esculenta* (p. 138), *Rana lessonae* (p. 148) and *Rana ridibunda* (p. 154). In his comments of the former map, R. GÜNTHER (p. 139) rightfully writes: "*R.* kl *esculenta*'s range is almost identical with that of *R. lessonae*." However, comparison of the maps provided for these two species shows that they display considerable differences: the first species is reported from 1172 squares and the second from 767; furthermore, the reverse situation also exists (*R. lessonae* but not *R.* kl. *esculenta* being reported for some squares), so that on the whole the overlap between the two maps is less than 767 squares. Strictly taken, these data would suggest that in more than 35 % of the squares where it is present (405/1172), the klepton *R.* kl. *esculenta* occurs there without *R. lessonae*. In part of these, *R. ridibunda* is also reported, but in most of them *R.* kl *esculenta* alone is shown, so that in these squares, according to the *Atlas*, green frogs appear to be represented only by pure populations of *R.* kl. *esculenta*. Such populations are indeed known to exist, especially in central and northern Europe, but it is unlikely that they occur in all the squares where *R.* kl. *esculenta* alone is reported in the *Atlas*. Particularly striking in this respect are all the spots on the map of *R.* kl. *esculenta* in southern and south-western France, as is aptly underlined by R. GÜNTHER in his accompanying text (p. 139) Looking at the map of p. 138 gives the misleading impression that the distribution of *R.* kl. *esculenta* virtually covers all the territory of France, and may be limited in the South-West by the chain of the Pyrenees. Actually, all published evidence available for the time being suggests that both *R. lessonae* and *R.* kl. *esculenta*, at least as natural populations, are absent in south-western France, and are replaced there by the "P-G system", i.e. mixed populations of *Rana perezi* and *Rana* kl. *grafi* (see CROCHET et al., 1995) In this respect, the *Atlas* seems much more reliable on the Spanish than on the French side of the Pyrenees. This may be due to different methodologies followed by the national coordinators of the inventory in these two neighbouring countries. The data for France closely resemble those published in the French *Atlas* (CASTANET & GUYÉTANT, 1990: 86), which were clearly unsatisfying as the different kinds of green frogs had not been distinguished by most observers. In this respect, R. GÜNTHER is fully correct when he writes in the *Atlas* (p. 139): "records of occurrences by inexperienced people in questionnaire actions are doubtful, because water frogs are difficult to identify". However, this comment has a much wider reach than it seems from this modest sentence, as green frogs are not the only species difficult to identify for "inexperienced people", and data in the *Atlas* concerning some countries (such as France) came mostly from "questionnaire actions".

The maps provided in the *Atlas* for *R.* kl. *esculenta*, *R. lessonae* and *R. ridibunda* are therefore most unreliable and cannot be used for scientific analysis. In such a case, rather than mapping separately these three species, and even without going into the details of the nine population types that can be recognized in these frogs (see e.g. RYBACKI, 1995: 346), it would have been useful to present at least five maps: two of "L-E system" and "R-E system" populations (UZZELL & BERGER, 1975), and three of pure *R. lessonae*, *R. ridibunda* and *R.* kl *esculenta* populations. Of course, such maps could not be prepared by "inexpe-

rienced people", and could be so only on the basis of laboratory work or of bioacoustic survey by experienced researchers: the total number of spots that could currently be obtained this way would be much lower than that presented in the *Atlas*, but this would be "better than nothing" We here touch a basic methodological question regarding this kind of atlases, which will be considered in more details below: what is "better than nothing"? Is it a nice but unreliable map covered with hundreds of spots, or a reliable map with only a few dozen spots based on scientifically reliable data?

Several other green frog maps are open to the same questions. The distinction between *R. ridibunda* and *R. perezi* also requires good experience or laboratory techniques, so that the parts of the maps in the areas where both species are stated to occur (southern half of France) are also highly doubtful. Strangely, while the *Atlas* took a lot of information from questionnaires as granted, it did not include many data already published by professionals and based on reliable laboratory techniques. Thus, although they were only recently given Latin scientific names (DUBOIS & OHLER, 1995*a*; CROCHET et al., 1995), beside *R.* kl. *esculenta*, two other kleptons have been known for a long time to occur in Europe, and published data are available about their distribution (GÜNTHER, 1979; GRAF & POLLS PELAZ, 1989; OGIELSKA et al , 1995): however, in the *Atlas*, distribution data for one of them (*Rana* kl. *hispanica*) are lumped with those of *R.* kl. *esculenta*, and those available for the second one (*Rana* kl. *grafi*) are completely ignored

The *Atlas* is supposed to provide information on introductions, and such data are important indeed to point out the potential problems of genetic and faunistic pollution alluded to above. However, establishing that a population is of alien origin deserves careful work and information, and the *Atlas* is also disappointing in this respect. The introduced populations of *R. lessonae* in southern England mentioned on p. 149 in R. GÜNTHER's text are not shown on the map of p. 148, and the introduced populations of *R. catesbeiana*, *R. lessonae*, *R. ridibunda* and *R.* kl. *esculenta* reported by ARANO et al. (1995) are ignored in the respective maps of these species, despite the fact that these data are referred to on p. 153 in the text on *R. perezi* by M. GARCÍA PARÍS. On the other hand, no reference or comments are provided for the statement that some populations from Italy and Denmark are composed of or derived from introduced specimens of "*Rana balcanica*", i.e. of *Rana kurtmuelleri* or, if the latter species is not valid, of *Rana ridibunda*. Inversely, no reason is given for considering only two of the three spots credited to the snake species *Natrix maura* in Corsica as introduced, although all observations of this species in this island were reported in the same publication (FONS et al., 1991) and are most probably the result of introductions from Sardinia (DELAUGERRE & CHEYLAN, 1992, 84).

Let us now leave the green for the brown frogs, and look at the map of *Rana dalmatina* (p. 134). This map shows a continuous presence of this species all along the Pyrenean chain, except in the French eastern part of the chain. This information is highly surprising, and would call for an explanation, but K. GROSSENBACHER, in his accompanying text (p. 135), does not discuss it in detail, just writing that "old records from Cataluña could not be confirmed in recent years". However, DUBOIS (1982*c*, 62-64) provided a detailed analysis showing that, although this species is present in the Landes and in the Garonne valley, and can probably reach the foot of the chain, no serious data are available to ascertain its presence in the Pyrenean chain itself, at least on its French side. He pointed to several misidentifications by previous authors and suggested that most, if not all, of the older reports of this species in the chain were based on specimens of *Rana temporaria*, particularly of long-legged specimens which he proposed to call provisionally "Gasser's frog" On the basis of an extensive survey of 3220 publications (i.e. roughly one third more than all those cited in the European *Atlas*) dealing with the chorology of the French herpetofauna (PARENT, 1982), PARENT (1981) proposed a distribution area of *R. dalmatina* excluding three French Pyrenean departments (Basses-Pyrénées, Hautes-Pyrénées, Pyrénées Orientales) and including two other ones (Ariège, Haute-Garonne), but possibly on the basis of extra-Pyrenean populations. A few pieces of evidence support the idea that the range of the species extends to the first foothills of the chain, at least in some areas: ZUIDERWIJK & VEENSTRA (1984) reported the species from several localities of the Basque provinces of Alava and Navarra south of the Pyrenean chain proper, and Pierre-André CROCHET (personal communication) has seen typical eggs, larvae and adults of *R. dalmatina* in Ariège (Plantaurel chain). I personally had the opportunity to see only one specimen of "agile-like" frog in the Basque country, with Annemarie OHLER and Miguel VENCES in 1997 in a locality where a local naturalist had reported having seen *R. dalmatina*: this specimen (shown here in fig. 1) is a long-legged Pyrenean *R. temporaria*, i.e. a Gasser's frog (DUBOIS, OHLER & VENCES, unpublished data). I know of no other convincing published data or specimens supporting the presence of *R. dalmatina* in the French Pyrenees.

Fig. 1 – Specimen (MNHN 1997.4446) of long-legged *Rana temporaria* ("Gasser's frog") from along Hasquette river north of Hasparren, Pyrénées-Atlantiques, France, 28 October 1997 (photo Pierre-André CROCHET).

However, and without any discussion, CASTANET & GUYÉTANT (1990) mapped the species as present in several parts of this chain, even at high altitude (which is quite impossible), and their data seem to have been uncritically incorporated in the European *Atlas*. It is likely that most, if not all, of the Pyrenean spots credited to *R. dalmatina* in the latter book are based on observations of *R. temporaria*. Nevertheless, the state of the art concerning our knowledge of the amphibians of this region (see DUBOIS, 1982*c*, 1983*a*; SERRA-COBO, 1993) is not such that the presence of *R. dalmatina* at low elevations in the Pyrenean chain can be completely ruled out. This is a typical example of a situation where, if correctly carried out, an international inquiry could bring interesting new data But, to be useful in this context, the data received from questionnaires should be carefully evaluated. we here touch basic methodological questions that will be discussed in more detail below.

Absence of distinction by some observers between *R. temporaria* and *R. dalmatina* raises other problems. The general distribution areas of both species widely overlap in western and central Europe, as it clearly appears on the maps of p. 134 and 158 of the *Atlas*. However, all those who have field experience know that, at least in western Europe (e.g. in most of France), both species are only rarely found together in the same localities, even in plain habitats which look superficially quite similar: for example, in the Paris

region, *R. dalmatina* alone is found in the Fontainebleau forest and *R. temporaria* alone in the Carnelle forest, and, in forests where both occur (e.g., the Rambouillet or the Compiègne forests), they breed together in some ponds only, while others only harbour one of the two species (DUBOIS, MORÈRE, OHLER, PAYEN & VACHARD, unpublished data). Careful analysis of such facts would allow to know better the histories and ecological requirements of both species, and to be more efficient in our conservation strategies. In western France, each species seems to be absent from rather large areas where the other one is present (DUBOIS, unpublished data), and a careful mapping of the occurrence of both species would be most interesting and useful. However, it is clear that, if and when someone wants to undertake such a study, the latter should be started from the beginning, as the data of the two French and of the European atlases are not reliable: even if the number of spots based on misenditifications between the two species is low, there is no way for the reader to know *which* spots are wrong, as no voucher specimens can be re-examined. In such areas, the European *Atlas* will be of little help to solve scientific questions and to help taking decisions regarding conservation matters.

CONSERVATION PROBLEMS

One of the stated purposes of this *Atlas* is to serve as a source for information on conservation problems facing European amphibians and reptiles. A brief introduction to this question by K. CORBETT is provided at the beginning of the book (p. 29-30), and the third appendix of the volume, by M. E. OLIVEIRA, P. DASZKIEWICZ & B. GAUVRIT (p. 408-412), presents data on the conservation status and the level of threat of each species in Europe, under the form of a table giving their coded categories in the species lists of the Habitat and Species Directive, of the Bern Convention, of the CITES Convention and of the IUCN Red Lists. Most unfortunately, only the codes of the categories are provided in this table, without their definitions or descriptions, and even without bibliographic references to such information, so that this table will be of little help to many users of the *Atlas*.

By themselves, chorological data on current and past distribution of species can be a precious help regarding the recent evolution of their population status and a guide for future conservation actions, but of course, to be so usable, these chorological data must first be reliable. Furthermore, additional problems must be considered, among which two are of particular importance and will now be discussed: (1) the need for a good knowledge of the existing literature, particularly concerning old data on distribution and population status of the studied species; (2) the taxonomic and genetic heterogeneity of species, which results in the particular significance and importance of threats on some populations.

The first problem will be discussed in the light of the example of the species *Pelobates fuscus*. The map presented in the *Atlas* (p. 110) shows a rather "logical" distribution, reminding in many respects those of other species, such as *Bufo viridis*: according to this map, *P. fuscus* appears to be a species widely distributed and apparently common in eastern and central Europe, but whose distribution ends quite abruptly west of the Alps, of Lorraine and of eastern Benelux. The author of the accompanying text for this map, A. NÖLLERT (p. 111) seems to consider this map to show, not only the current situation of populations of this species in western Europe, but also its "potential" area of distribution in this region, since he writes that Alsace and eastern Benelux are the "western distribution limit" of the species. With such ideas in mind, he is rightfully surprised by the presence in the *Atlas*'s map of an isolated spot in central-western France, and he writes about it: "Another isolated (doubtful?) locality is situated in Central France (Buzançais) " This isolated spot, which was also the only one shown west of Lorraine in the French *Atlas* (CASTANET & GUYÉTANT, 1990), was based on three tadpoles reported by DUBOIS (1984*c*), who discussed the status (introduced or not) of this population and specified that these tadpoles were kept in the collections of the Paris Museum under the numbers MNHN 1984 448-450 If the authors and editors had "doubts" about this observation, which, quite unlike most other data of the *Atlas*, was accompanied by voucher specimens, why didn't they examine them? Posterior to the 1984 observation, mating calls of the species were heard in the same locality on 15 April 1985 (DUBOIS, 1985*b*, unpublished data), and an adult female and a young one photographed (fig. 2), and then released, on 2 May 1986 (DUBOIS & EVRARD, unpublished data), so that the population is known to have existed in this locality at least until 1986. But this is not the most important point in this case.

Fig. 2. Specimen (released) of *Pelobates fuscus* from a small pond near Sainte-Gemme, Indre, France, 2 May 1986 (photo Philippe EVRARD).

Although DUBOIS's (1984*c*) observation is the only one from northern France mapped in CASTANET & GUYÉTANT (1990) and in the European *Atlas*, these data are not the only recent ones from this region: MORÈRE (unpublished lecture cited in DUBOIS, 1984*c*) reported survival of the species in several other French localities, but unfortunately he never published these important data However these recent data are nothing compared with the numerous older data, especially from the 19th century, documenting the presence and distribution of *P. fuscus* in northern France. On the basis of a critical survey and evaluation of the existing literature (PARENT, 1982), PARENT (1981) synthesized the then available and reliable data· he listed the species as having been reliably reported in 16 departments of northern France and doubtfully in 17 additional ones. On the basis of these data, he mapped the southern limits of the 19th century distribution of this species in northern France. He also stressed the fact that this species was currently suffering regression in Belgium. Regression of populations of this species in France is an important fact during our century. One hundred years ago, *P. fuscus* was a rather common species in northern France, even close to Paris, where it was repeatedly reported by such famous batrachologists as DUMÉRIL & BIBRON (1841· 480), LATASTE (1876: 12), HÉRON-ROYER (1886: 75-76) or BOULENGER (1897: 203-204), and from where specimens are kept in the collections of the London Museum (BOULENGER, 1882 438, 1898: 346), of the Paris Museum (MNHN 4551, Bondy, 19 April 1875, coll. DEGUÉ; MNHN 8063, 8066, CD.56, neighbourhood of Paris, no date) and of the Wien Museum (NMW 6567, Paris, 1879, coll. LATASTE), but now this species seems to be totally extinct in all the Paris region (see DUBOIS & OHLER, 1988). Regression, and in fact almost total extinction, of *Pelobates fuscus fuscus* in northern France during our century, while the same subspecies seems to have remained quite healthy in central and eastern Europe, is a major fact that (1) calls for a scientific explanation and (2) should have been stressed in a distribution *Atlas* of European amphibians. This appears indispensable in order to allow this fact to be properly taken into account in international conventions and other official lists, all documents which until now have ignored

the high threat level of this subspecies in this part of its range, while much more attention was paid to the subspecies *Pelobates fuscus insubricus* in northern Italy: thus, in CORBETT's introductory chapter to the *Atlas*, in the appendix by OLIVEIRA et al. and in CORBETT's (1989) book, only the latter subspecies is mentioned regarding conservation matters, and the subspecies *Pelobates fuscus fuscus* is not even cited, although it is clearly the most endangered taxon of the whole French amphibian fauna. Probably the second species in this respect is *Bombina variegata*, many populations of which have become extinct in several parts of France during the last decades (BREUIL & PAILLETTE, 1983, DUBOIS, unpublished data), but this fact also is completely ignored in the European *Atlas*. Both species *P. fuscus* and *B. variegata* are in France in the westernmost part of their range, which may in part account for their fragility in these regions.

This example shows that, to be useful for dealing correctly with conservation problems, a distribution survey cannot rely only on recent data, but should also incorporate a good (i.e., not only complete, but also critical) knowledge of all the older literature and collection data. The qualification of "critical" is important. In systematics and faunistics, like in many other scientific fields, some mistakes can have a very long life, through their repeated copy from the original publication to a second, then a third one, etc. It may be difficult to break such chains of repetitions, as is well exemplified again in the *Atlas*: in his text on *Algyroides fitzingeri* (p. 219), once again B. SCHNEIDER gives credit to the "legend" of the islets Bocognanco, Cauro and Orezza, although the latter have never existed, as was already stressed by LANZA (1983: 733) and DELAUGERRE & CHEYLAN (1992: 66). Critical analyses of the data in the older literature are therefore of great importance. Probably, for the time being, the most thorough survey of the chorological herpetological literature in Europe is that of PARENT (1981, 1982) for France and Benelux, which unfortunately has not been duly taken in consideration by authors and editors of the European *Atlas*. Hopefully this important work will be consulted by future workers on the European amphibians and reptiles, and hopefully also similar works will be prepared and published concerning other parts of Europe. Such serious and critical surveys of available older data, and also of museum and other herpetological collections, will be the only way to have objective information on the past distribution of species in Europe, and, by comparison with recent data, to obtain reliable estimates of the recent changes in the status of the populations and species, and of threats hanging over them. In the absence of such objective information, part of the decisions regarding conservation of the European species (inscription on official lists, allocation to threat levels, legal restrictions to their transport or commerce, etc.) will be based exclusively on subjective "feelings" by a few people, so that only the species which happen to be personally well-known of these persons will be properly dealt with, while others, like the northern French populations of *Pelobates fuscus* just discussed, or some populations of *Triturus alpestris* discussed below, will be ignored or their status and threats will be incorrectly assessed.

The second example is meant at stressing the fact that species are not "black boxes" of identical individuals or populations, but display internal variability, and particularly geographical variation that may in some cases be worth of being highlighted through taxonomic recognition of subspecies. In such cases, in the frame of an international conservation policy, special attention should be paid to some subspecies having a very limited distribution and/or being particularly threatened with extinction. The newt species *Triturus alpestris* is a good example of this situation. Although the distribution of this species, as illustrated on the map of p. 72 of the European *Atlas*, covers a large part of western, central and southern Europe, this distribution shows discontinuities, and several subspecies are currently recognized within this species. The conservation status of these different subspecies is not the same, and this fact must be taken into account when considering legal and administrative decisions. The nominative subspecies *Triturus alpestris alpestris* has a very wide distribution area with numerous populations. as such, this subspecies is not particularly threatened with extinction, although, like all other European species, destruction or modification of aquatic habitats clearly results in the regression or extinction of many local populations. But the situation is much more critical for other subspecies currently recognized in this species. Thus, A. ZUIDERWIJK is correct when she writes, in her accompanying text of *T. alpestris* (p. 71): "The subspecies *T. a. inexpectatus* is rare and endangered and any collecting of specimens means a serious threat to this subspecies." Actually, this subspecies, which some consider to deserve species status as *Triturus inexpectatus* (BREUIL, 1983, 1986, ANDREONE, 1990), is currently known from only four populations (DUBOIS & BREUIL, 1983; DUBOIS, 1983*b*, 1993; GIACOMA et al., 1988), some of which are threatened with extinction (DUBOIS, 1983*b*), and the absence of any mention of this subspecies in all current official lists of the European fauna (see OLIVEIRA et al.'s appendix to the *Atlas*) is a serious lack,

for which the SEH Conservation Committee clearly has some responsibility. This subspecies is not at all mentioned in CORBETT's introductory chapter to the *Atlas* or in CORBETT's (1989) book. Other subspecies of *T. alpestris* also deserve more attention than they are given in the *Atlas*.

Of particular importance for amphibians, especially for species or populations that spend a large part of the year in water, are the problems posed by the introduction of fish in closed water bodies (see e.g. DUBOIS, 1990, 1991, 1994). For several European amphibian species, this factor of population's regression or extinction is certainly as severe as, if not much more so than, "persecution" or "predation by domestic cats", but, contrary to the latter, it is not even once mentioned in CORBETT's text in the *Atlas* or in several species accounts where they should have been so, like *Triturus alpestris*. The importance of this threat on some amphibian species seems therefore to be underestimated by several European herpetologists, and may then deserve a special discussion. Species of the genus *Triturus* are particularly vulnerable to this factor, especially in their populations where newts spend most or all of the year in the water, like many mountain populations. This can be highlighted by several examples.

On 22 August 1978, I had the opportunity to visit the Prokosko jezero (Bosnia-Herzegovina), type-locality of the nominal subspecies *Triturus alpestris reiseri*, and I saw thousands of these large-headed newts standing on the bottom in the clear water, at the rate of several ones per square meters all around the lake (DUBOIS, unpublished data), but I was not allowed by the guards of the lake to collect even a single specimen: although disappointed, I was satisfied with the impression that this unique population was carefully protected. I informed Michel BREUIL, who applied for and obtained an official collecting permit for some newts, and visited again the locality in September 1981 and August 1982, but had then the bad surprise (BREUIL, 1985) to realize that, seemingly as early as in 1972, trouts had been introduced in the lake, and that the type-population of this nominal subspecies was almost extinct, just a few specimens having escaped trout predation in a few small zones of difficult access or in neighbouring small ponds. BREUIL (1985) described similar situations for many other mountain populations of *T. alpestris*, including the type-populations of several other nominal subspecies, and BREUIL & PARENT (1988*a-b*), in their interesting study (not cited in the *Atlas*) of the taxonomic, distribution and conservation status of the subspecies *Triturus alpestris veluchiensis*, insisted on the dangers that could result for this subspecies from salmonid introductions. Similar threats have concerned *T. alpestris* in the French Alps, e.g. in the Parc National des Ecrins, where introduction of trouts was followed by total extinction of some populations (BREUIL, 1985). This problem is there of a particular significance, since in this area the subspecies *Triturus alpestris alpestris* and *Triturus alpestris apuanus* meet (BREUIL, 1986): extinction of natural populations following trout introductions will preclude any further study, e.g. by protein electrophoretic methods or by study of DNA microsatellites, of fine genetic structure of these interesting populations, to reconstruct migration and introgression phenomena involving the two taxa.

For a species like *Triturus alpestris*, which often inhabits mountain lakes where most or all of the year cycle may take place in the water (especially in populations with a high percentage of paedomorphic specimens), introduction of fish may be a very rapid and irreversible factor of extinction of populations. The problem also exists for plain species or populations of newts, especially when associated with another threat factor, duly mentioned by CORBETT in the *Atlas* (p. 29), namely habitat fragmentation. In some parts of the Paris region for example, ponds and other breeding habitats suitable for amphibians have become so rare that many populations of these animals may be regarded as inhabiting continental islands completely isolated one from another, and particularly vulnerable. In a growing number of small isolated ponds, local people have introduced cyprinids, not for fishing purposes but apparently solely for the purpose of seeing red fish in the water: in a number of these ponds, these fish, probably through predation on the eggs, have led local populations of amphibians, and particularly of newts, to extinction, and as these populations are now separated from other neighbouring conspecific populations by impassable zones of monocultures, built areas or roads, they cannot be colonized again (DUBOIS, unpublished data). In such regions, newt populations may become extinct one after another, each local extinction contributing to weaken even more the remaining neighbouring populations and leading ultimately to complete extinction of some species over a growing area.

This important factor of fish introductions should therefore be given proper attention: diffusing information on this problem and trying to introduce in international and European legislative texts severe regulations against uncontrolled introductions of fish are among the first actions European batrachologists are entitled to expect from a European herpetological society, but unfortunately this question is not tackled even once in CORBETT's introduction to the European *Atlas*. Nevertheless, a number of European

batrachologists are aware of this problem, as is made clear by the fact that it is mentioned in passing by several of them in their accompanying texts in the *Atlas*, in the following species accounts: *Euproctus asper*, *Euproctus platycephalus*, *Triturus italicus*, *Triturus montandoni*, *Triturus vulgaris*, *Bombina bombina*, *Pelobates cultripes*, *Pelobates fuscus*, *Rana iberica* and *Rana temporaria*. This question is also briefly but repeatedly mentioned in several chapters of CORBETT's (1989: 15, 45, 130-131, 136, 139, 155, 160-161, 171, 175-176, 202, 208, 256) book about conservation of European amphibians also published under the umbrella of SEH. The absence of any general statement and international strategy of SEH regarding this problem is all the more difficult to understand. Possibly the old tradition of always considering amphibians and reptiles together, under the general discipline of "herpetology", may contribute to obscure the biological particularities of amphibians that require distinct conservation strategies for these animals. As a matter of fact, the particular problem posed to amphibians by fish introductions was among the examples mentioned to support the need for recognizing batrachology as a distinct scientific discipline (DUBOIS, 1991).

METHODOLOGY OF THE INQUIRY

The fact that in the *Atlas* some of the spots credited to *Rana dalmatina* were almost certainly based on observations of *Rana temporaria* shows that the critical evaluation of data before their incorporation in the maps was insufficient. These two species show superficial resemblances but nevertheless any experienced naturalist can distinguish one from another by simple examination of the external phenotype (DUBOIS, 1984*b*). If an observer providing basic data to the inquiry cannot tell *R. temporaria* from *R. dalmatina*, there is a strong possibility that the same observer will also have identification difficulties in many other cases, such as all other frogs of the genus *Rana*, *Triturus helveticus* versus *Triturus vulgaris*, *Bufo calamita* versus *Bufo viridis*, *Hyla arborea* versus *Hyla meridionalis*, or even *Alytes* versus *Pelodytes* or *Pelobates*, or *Discoglossus* versus *Rana (Pelophylax)* - not to mention the lizards.

Such a statement can easily be confirmed by any zoologist who has examined numerous museum collections: no major collection worldwide is free from specimens badly identified, even if the work was done by professional scientists. To mention here only examples from the Paris Museum collection, which has had a continuous curating by professional zoologists since 1793, here is a non-limitative list of identification mistakes concerning western Palearctic species which I or other colleagues found while cursorily looking into the collections since 1977: *Rana temporaria* under the names of *Rana dalmatina* (MNHN 1971 343; see DUBOIS, 1982*c*: 63), of *Rana ridibunda perezi* (MNHN 1973.64-67; see DUBOIS, 1982*c*: 63) or of *Rana* gr. *esculenta* (MNHN 1987.832-914); *Discoglossus pictus scovazzi* under the name of *Rana ridibunda perezi* (MNHN 1961.52, 1961.56, 1961.58, 1961.70-71); *Alytes obstetricans maurus* under the name of *Discoglossus pictus* (MNHN 1908 111, 1994.1894-1897; see DUBOIS, 1998*a*). Similar gross mistakes can be found in the Paris Museum collections from other countries, and actually in most large herpetological collections all over the world. At least, museum collections have an important advantage over other kinds of data: specimens remain available for study, re-examination and correction of identification. This is not the case of distribution data based on field observations for which no voucher specimen was kept: in such cases one is bound to rely on the validity of the identifications made in the field by observers. We all know examples of gross identification mistakes made by people not closely acquainted with the zoological group considered, or even by people who should, according to their responsibilities, avoid such errors. Such problems are not new: identifications of specimens by a number of authors of the past, who had a particularly bad knowledge or "feeling" about amphibians and/or reptiles, cannot be taken for granted, and, before using their data, their specimens must be examined again. Several names could easily be mentioned in this respect, and are well-known of all experienced taxonomists.

These examples are not given in order to throw "shame" on any particular persons, but to really stress, for the many people who do not seem to be aware of this problem, how the identification of European amphibians (and reptiles) may in many cases be difficult without proper feeling, training, experience and sometimes sophisticated techniques. Some think that this problem can be solved by the publication of books and identification keys aiming at helping "amateurs" (or some "professionals") to

recognize the species, or by special training courses like those organized by some herpetological societies in Europe. This is certainly in part true. However, several of the existing books contain mistakes of various magnitudes, and training courses are useful only if organized by truly competent naturalists. But this may not be the most severe problem: a 35-year experience has convinced me that identification of many of these animals requires, beside theoretical knowledge, a certain amount of "feeling" that some people will always lack. This statement will be well understood by all good field naturalists, who know that no book or training course will ever replace the intuitive knowledge of some people in the field, who will immediately know where to go to look for certain mushrooms in a forest or certain marine animals at low-tide, even if they are unable to "explain" how they found them, while others will spend the full day with them but find nothing.

In a sense, books, field guides and keys may play a rather *negative* role. Providing seemingly simple keys using just a few characters may give inexperienced people the misleading impression that identification of European amphibians and reptiles is a simple and rather mechanical process, rather than a *scientific action*: "Actually, putting a Latin name on a specimen is a *scientific*, not technical, activity. Giving a name amounts in fact to making a *scientific hypothesis*, that of conspecificity of this specimen with the one that originally beared this name, i.e., in nomenclatural terms, its *name-bearing type*" (DUBOIS, 1998*b*). Rather than a single-step process based on a few characters, identification of a specimen must rely on a synthetic appraisal of all characters (phenotype, behaviour, mating call, etc.). The existence of "keys" may contribute to perpetuate a typological conception of species, according to which intraspecific variability is ignored or grossly underestimated. There are few "diagnostic" characters that are not liable to vary within a species, and most of the characters used in identification keys can in some cases be misleading. Here are a few examples, all based on my personal observations, of such "diagnostic" characters of European amphibian species that may vary in some individuals or in some population and might lead to uncorrect identifications by inexperienced observers: in *Rana temporaria*, although usually the leg is shorter than in *Rana dalmatina*, in some populations (Gasser's frog and *Rana temporaria honnorati*) it may be almost as long as in the latter species, the heel extending beyond snout tip when the leg is folded along the body (DUBOIS, 1982*c*); in *Hyla arborea*, although usually a dark stripe is present on flank, this stripe may in some individuals be very weak or absent, like in *Hyla meridionalis*; in the genus *Bufo*, although usually a yellow mid-dorsal stripe is present in the species *calamita* and absent in the species *viridis*, exceptions to these "rules" can be observed in some specimens or populations of both species; in *Alytes obstetricans obstetricans*, although usually three tubercles are present on palm of hand, rare individuals may have only two tubercles, like in *Alytes cisternasii*; in *Triturus alpestris*, although usually the throat is unspotted in the subspecies *alpestris* and spotted in the subspecies *apuanus*, it can be spotted in some individuals and particularly in some populations of the nominative subspecies, similarly, *Triturus helveticus*, unlike *Triturus vulgaris*, normally has an unspotted throat, but some individuals may have black gular spots, usually surrounded with white. In all these cases correct identification of specimens generally raises no real problem if the phenotype is considered as a whole and not as a collection of artificially isolated "diagnostic" characters.

All the seemingly pessimistic statements above are not meant at stating that all information from questionnaires should be banned from a distribution inquiry, but that such data should be used with considerable caution and after critical analysis. In other word, in order to carry out an international distribution inquiry, a serious reflection on methodological matters is in order. Let us now examine more closely these methodological questions[1].

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1. In all what precedes, I have assumed that, if identification mistakes were made by some observers, they were so involuntarily, but this may not be the case, as stressed by Frank GLAW (personal communication, 16 January 1998) in his comments on the manuscript of this paper: "Beside the incompetence of observers there are some other aspects to be considered: people can consciously provide wrong data, for example for political reasons. They may state that endangered 'red list species' occur in a given habitat just to have better arguments to protect 'their habitats' as nature reserves. However, it is nearly impossible to find hard evidence for such kind of fraud. It is even possible that people introduce specimens from another locality or that they provide voucher specimens with wrong locality data. Biological inventories provided by commercial bureaus sometimes seem to produce their species lists just by looking at the habitat. They then write down species that 'must' occur there (like *Rana temporaria*), although they were actually never found. Another problem is that of 'psychopaths', who try to make themselves interesting by providing rather spectacular data. And of course even voucher specimens can be very misleading when locality data are confused. Such voucher specimens with wrong locality data can produce

Any distribution inquiry makes sense only if spots are based (1) on correct identification of observed specimens and (2) on exact and precise locality (and, although less importantly, date) of observation. Any reflection on the methodology of such an inquiry should therefore start with a careful evaluation of the problems posed by the *scientific validation* or control of these basic data. This is indeed what is found in serious methodological reflections on distribution inquiries (e.g. PARENT, 1974: 81-88, 1976, 1979. 10-15, 1981: 86-87, 1982. 373-390; ALCHER et al., 1979, BREUIL et al., 1982; DUBOIS & MORÈRE, 1983; DELAUGERRE & CHEYLAN, 1992: 16-17) Surprisingly, this question does not seem to have been at the center of the reflections of the SEH Mapping Committee. The presentation of methodological aspects of the *Atlas* by H. MAURIN et al. (p. 11-16) is very enlightening: it only deals with *technical* matters of coding information in questionnaires, of optical reading of the latter and of computer processing of the data leading to the building of maps, but almost nothing is said about scientific control of the data. The few words mentioning this aspect are very vague: "According to the objectives set by the Mapping Committee and also because of the way the work was organised, based upon a network of responsible persons, but also of 'loose' collaborators, it was necessary to choose a very simple methodology. This methodology had to be as free as possible from any language problems and well adapted to the type of available data." (p. 13). "Co-ordinators regularly sent the filled in questionnaires for processing. These were checked and then digitised When questionnaires were sent directly to the SFF/SPN by collaborators, these were first sent back to co-ordinators for approval before being entered in the computer " (p. 15) Therefore, it appears that scientific control of the validity of the data computerised and used for drawing the maps was not cared for by the Mapping Committee but by the national co-ordinators chosen by this Committee. The Mapping Committee does not seem to have prepared guidelines for this scientific control, so that each national unit of the inquiry was apparently free of developing its own scientific methodology. Viewed under this light, this international inquiry therefore appears more like the *technical* juxtaposition under a single mapping system of several distinct inquiries having slightly or strongly different *scientific* methodologies for the collect and scientific control of the basic field data. Heterogeneity in the scientific reliability of the results is not surprising under such a methodology.

A similar lack of concern and information on methodological problems of scientific control of the validity of the observations is also striking in various other texts presenting the SFF, and later SPN, working methodology (DE BEAUFORT & MAURIN, 1985; MAURIN, 1989, 1994; MAURIN et al., 1993) or the SHF inquiry on the distribution of amphibians and reptiles in France which has largely served as a model for the European inquiry (CASTANET, 1978; CASTANET & GUYÉTANT, 1990; GASC et al., 1994). DUBOIS (1982*a*) and DUBOIS & MORÈRE (1983) have shown that the methodology of the latter inquiry (by a posteriori "global validation" of computer-produced maps, with simple suppression of some "unlikely" spots without going back to the original questionnaires, rather than by a priori "spot by spot" critical control of basic data) was scientifically unsatisfying. A different, stricter methodology was advocated by several batrachologists, first within (ALCHER et al , 1979), then outside (BREUIL et al , 1982) the SHF inquiry, but this new inquiry did not result in a final publication, for lack of financial support and staff, and despite the fact that about 3500 amphibian distribution data from France had been gathered (PAYEN & DAUM, 1988); rather than lose all these data, it would now seem logical to incorporate them in the computerised data base of SPN and SEH, but before doing so, it may be useful to discuss in more detail these methodological questions.

Technical problems of collect, computerisation and mapping of the data are of course important for any enterprise of the magnitude of the European *Atlas*. But these technical questions should not obscure the scientific ones. As everybody knows, computers will only give you back what you have fed them: if the basic data (spots of observation of species) are wrong, the final maps will be incorrect, misleading and unuseful How can reliable scientific distribution data on organisms be obtained? Zoologists have worked for two centuries for ascertaining the geographic distribution of species and mapping them, well before the introduction of computers, data bases and automatic mapping. Two major methods were used in this respect: field observations, and capture, fixation and conservation of specimens in permanent collections. Clearly the second method is the only one that meets the requirements of scientific research: in all fields

much confusion (there are numerous examples from Madagascar), since they are generally considered to be more reliable than the observations of any observer " These very justified comments provide an additional reason for paying a close attention to methodological questions in any collective inquiry.

of science, *repeatability* and *independent evaluation* of data by different researchers is a prerequisite for acceptability by the scientific community of these data as genuine scientific results. Since distribution data are not only geographic data, but also historical ones, repeatability of observation is not possible later if no voucher specimens, or at least photographs, paintings, drawings or detailed descriptions, are available. What can we do with old data when no specimen or precise information about them was kept? If we do not want to just discard these data, which may play a crucial role in some cases as testifying to the past presence of a species in a region where it is now lacking, we are bound to evaluate the reliability of the old observation through (1) evaluation of the risk of taxonomic mistake at the time of the observation and (2) evaluation of the taxonomic competence of the observer.

The risk of taxonomic mistake is of course much larger when several similar species are likely to occur in the area of the observation: if this is the case, and if particular characters are of importance for the correct identification of species, it will be useful to see if the observer mentioned having checked these characters in the reported specimens. But, of course, the problem will be almost insoluble if the taxonomy has changed since the time of the observation: if several species are now recognized in what was then believed to be a single species, and if several of these species may be expected to have lived in the observation's locality, it will usually be impossible to allocate a posteriori this observation to a species, and the spot must be abandoned altogether, at least at species level (it may remain as an evidence of the occurrence of an unidentified species of a given genus or species-group).

As for the taxonomic competence of the observer, this point is rarely stated in full words in scientific publications, perhaps because it sounds "politically incorrect". However, it is a reality, and science, if it is to remain a reliable reference for the knowledge of reality, cannot accept all data in order not to upset anybody. As tackled above, all taxonomists know that not all their colleagues are similarly reliable in their identifications. In most cases, I personally will have no hesitation (except when there has been a recent change in the taxonomy of the group, as just mentioned) to accept field identifications, even when not documented by voucher specimens, from confirmed field naturalists like L.-F. HÉRON-ROYER, F. LATASTE or G. A. BOULENGER, but I will be much more careful with data from P. CHABANAUD (who could e.g. identify a Pelobatidae as a Bufonidae: see DUBOIS, 1980: 174; see also PARENT, 1976, 1981: 86), P. CANTUEL (see e.g. PARENT, 1981: 86, 1982: 82) or E. AHL (who could e.g. describe the same species as new under 10 different names: see GORHAM, 1974: 157). Even when very good naturalists are at stake, prudence may be justified, for example when identifications were based on tadpoles or on mating calls: a famous case is that of the albino tadpole, first identified as *Pelodytes punctatus* by LATASTE (1878), which was an *Alytes obstetricans* (HÉRON-ROYER, 1878, 1887; LATASTE, 1880). Even the great BOULENGER was not free from mistakes, since, unlike HÉRON-ROYER, he refused to accept the validity of taxa which are now recognized as valid under the names *Pelobates fuscus insubricus*, *Discoglossus pictus auritus*, *Hyla meridionalis* or *Rana temporaria honnorati*. The conclusion of all this discussion is that the greatest care should be taken before using field data undocumented by voucher specimens. Of course, in areas or countries poorly explored and for which data are scarce, mapping of amphibians may be in part based on sighting of specimens in the field even without capture, or on recording or hearing of mating calls (see e.g. DUBOIS, 1974, AMIET, 1983), but this can be done only by experienced naturalists, and usually, even for the latter, it is much more reliable to catch and examine the specimens in the hands and to keep them for further laboratory study.

Does this mean that data obtained from questionnaires are totally unreliable and that the thousands of data gathered this way for the European *Atlas* should be completely discarded? I am not suggesting this, but rather that control of these data should be much more careful, which is possible, as shown by some excellent distribution surveys published in the recent years. However, most of these works were of a much lower magnitude than the European *Atlas*. Careful control of the data is more realistic (which does not mean easy and quick!) in the case of surveys covering a much smaller geographical area (see e.g. the excellent distribution atlas of Corsican herpetofauna by DELAUGERRE & CHEYLAN, 1992) or only a given taxon (see e.g. the contributions of the *Catalogue of American amphibians and reptiles* published first by ASIH and now by SSAR, whose quality is due to a very careful, species by species, publication program). Of course, for a work of the magnitude (in terms of numbers of species and of observers, and of political heterogeneity of the geographical coverage) of the European *Atlas*, imposing stringent methodological requirements on the collect and treatment of data and on their analysis would have a cost, in terms of financial funding, of staff, of working time and of delays before publication. Whether or not this cost would be justified is another question that will be examined below.

How should the validity of observations be ideally controlled? A minority of observers do mention in some questionnaires the existence of additional information on a given observation report, such as photographs, drawings, descriptions or even voucher specimens (e.g., specimens found dead in the field): in such cases, any doubt on the identification is liable to be removed by study of these documents or specimens. But in the vast majority of cases no such additional information is available. In such cases, the only way to assess the scientific reliability of data is indirect. It can then rely on two major kinds of evidence: (1) an evaluation of the likeliness of the observation of a given species in a given area, or of the risk of misidentification in this given case, (2) an evaluation of the competence of the observer

Evaluation of the risk of misidentification requires knowledge of several important facts: whether the species reported in the questionnaire is sufficiently similar to another or several other ones to allow confusion by inexperienced observers; whether this confusability exists for all specimens or only for one sex or at some stages (e.g., egg, tadpole, imago, adult) or for some characters (e.g., mating call); whether in the area of the observation two or more such confusable species are likely to occur. This evaluation of risk should therefore be entrusted to specialists who should ideally have a good knowledge of both the region of the observation and of all confusable species likely to be present there. Finding such specialists may sometimes prove difficult In some regions or countries, there may exist for the time being no good specialist of some herpetological groups: in such cases, it may be necessary to entrust the responsibility of the local inquiry to someone from another region or country Reluctance to do so may be "politically" understandable but may result in poor scientific results.

Even more difficult, of course, is the evaluation of the competence of observers. For this, the best is clearly the existence of good personal contacts between the responsibles of the inquiry and the observers, ideally involving personal meetings and common field work. Contact can also be developed by mail or phone. Finally, if direct contacts are lacking, some evidence can be obtained from careful analysis of the questionnaires. Examining altogether all the questionnaires sent by an observer, before their possible distribution to species specialists or their computerisation, can be an efficient way to point to possible identification mistakes or difficulties. For example, if an observer sent numerous questionnaires from different localities in the Paris region mentioning the presence of *Triturus vulgaris* but none of *Triturus helveticus*, or the contrary, it will be likely that this observer did not distinguish both species, a similar warning of caution may come from seeing only *Rana temporaria*, but no *Rana dalmatina*, or only *Rana* kl. *esculenta*, but no *Rana lessonae*, in questionnaires from this region Various other kinds of information can be obtained through a detailed survey of all questionnaires sent by an observer, which can tell us a lot about the reliability of the data submitted A similar kind of evidence can be obtained, without seeing specimens, through detailed analysis of publications thus, a careful reading of the paper by SPITZ (1971) suggests that this author's report of *Lacerta viridis* and *Lacerta agilis* being often caught together in the same traps, in a locality where only the former species (rather now *Lacerta bilineata*, see RYKENA, 1991) is known to occur (J.-P. BARON, personal communication), was based on misenditifications where only male *Lacerta viridis* were recognized as such, while females were mistaken for *Lacerta agilis*.

What should be done when careful analysis of the data, under the lines suggested above, throws doubts on the validity of some identifications? The best is certainly not to simply "suppress" the data altogether, as the possibility always exists that a species, "unlikely" to occur in an area, was introduced in this region ignoring such data would result in losing an interesting information. If possible, direct contact should be taken with the observer, which will sometimes allow, through a discussion, to find the source of the problem. In some cases, it will even be possible to correct a posteriori an identification, so that the data will not be lost for the inquiry. Only in cases when doubts remain after this effort, should the data of the questionnaire be considered unreliable, and discarded before computerisation. But in such cases, the fact that a given observer misidentified some specimens should be kept in memory, and the possibility that other misleading data were sent by the same observer should be considered seriously, even if the other data by this observer "look reliable": we should always remember that, as much as an "unlikely" observation can be correct, a "likely" observation can be wrong.

This leads us to a final striking methodological problem. Data on the geographic distribution of animals on our planet are based on two major kinds of information: field collected specimens, with information on their collection date and place; and data based on scientific judgement, i.e. taxonomic allocation of these specimens. Only the second kind of information is liable to change with time: as taxonomy of a group evolves, or as misidentified specimens are re-examined, the names given to specimens may change But the specimens remain the same, and their place and date of origin also. When

a taxonomist re-examines a collection and changes some names, the corresponding specimens do not "disappear" from the chorological data, they only shift from one taxon to another: the spot on a map corresponding to a given specimen remains, only the scientific name associated to it changes. Furthermore, in any "professional" taxonomic, faunistic or chorological work, such re-allocation of names to specimens cannot be done "silently", it must be accompanied by a scientific justification given in full words when the change is introduced: new taxon, new synonym, correction of misidentification, etc. Just changing names of taxa on distribution maps, or mere "suppression" of some spots on the maps without written explanation, is not a serious scientific process. However, this is precisely what can be observed in the series of atlases published by SFF/SPN, of which the European *Atlas* is the last production.

Detailed comparisons of the successive maps provided for many species in the two successive versions of the French atlas (CASTANET, 1978; CASTANET & GUYÉTANT, 1990) and in the French part of the maps of the European *Atlas* show important differences. Not surprisingly, many of these differences are *increases* in the distribution assigned to a species: one expects such an increase as more and more data are collected. But other changes are the reverse way: for some species, the distribution in France recognized in these successive books shows a significant *decrease*. The only possible explanation of such facts would appear to be re-evaluation of the basic data and new taxonomic allocation of the spots to other species. But no written explanation of these changes were given with these successive versions of the maps. Let us consider the species *Rana arvalis*. To be sure, most of the spots credited to this species in the first French *Atlas* (CASTANET, 1978: 63) were completely outside the known range of the species (ARNOLD & BURTON, 1978; PARENT, 1981), and were most likely based on misidentifications. Suppression of these spots in the second version (CASTANET & GUYÉTANT, 1990: 82) is not surprising, but not a single word is provided to explain this: were these spots just erased, or transferred to other species after correction of identification? Concerning now *Bombina variegata*, an isolated spot north of Nantes has disappeared without any explanation between the maps in CASTANET (1978: 41) and in CASTANET & GUYÉTANT (1990: 58); in the European *Atlas* (p. 98), another unexplained suppression concerns an isolated spot in Normandy, although *Bombina variegata* was recently and reliably documented from this region by LEMÉE (in COLLEAU, 1986: 3). As for *Salamandra atra*, one of the two spots shown in CASTANET (1978: 23) has disappeared without explanation in CASTANET & GUYÉTANT (1990: 38), and all three spots shown in the latter map are absent in the European *Atlas* (p. 64). Other striking "silent" spot suppressions between the books of CASTANET (1978) and CASTANET & GUYÉTANT can be found in the maps of the species *Triturus vulgaris*, *Alytes obstetricans*, *Pelobates fuscus*, *Pelobates cultripes*, *Bufo viridis* and *Hyla meridionalis*; while in *Pelobates fuscus* and *Bufo viridis* the suppressed spots were indicated as "doubtful" in the first atlas, this was not the case for the other four species. The absence of any explanation for suppressions of spots from one atlas to the next one and of information on the fate of the "suppressed" spots (allocation to other taxa or complete discarding of the data) is not compatible with the claim that such atlases are scientific works: these changes are incomprehensible for the reader and, above all, as such undocumented changes have occurred already over three successive atlases, there is no reason to think that the next version of the European *Atlas* will not include new mysterious changes!

"BETTER THAN NOTHING"?

The European *Atlas*, a major collective international endeavour and realisation, is disappointing in its results, as the scientific validity of the basic data on which the maps were based is open to question. Clearly, some spots shown on the maps were based on erroneous identifications of specimens, some basic bibliographic references and museum specimens were ignored, and some texts contain important mistakes or omissions regarding either the distribution data or their interpretation, particularly in terms of conservation; additionally, this book contains a number of errors concerning taxonomy and nomenclature of European amphibians and reptiles. Although these weaknesses were clearly documented above, what is much more difficult to evaluate is their quantitative importance. When basic data are voucher specimens, which is the case for most taxonomic and distribution surveys of amphibians and reptiles over most of the planet (and particularly in tropical countries), mistakes can eventually be corrected whenever these specimens are examined again. But here the basic data are questionnaires, not

specimens. Whether these questionnaires will now be available to the international scientific community for critical study, as are usually museum specimens, is not stated in the *Atlas*. But, even if it is the case, re-assessment of the reliability of data in these questionnaires would be difficult, for several reasons analysed above. any questionnaire by itself may often be insufficient for this work, and additional information may be needed from direct contact with the observers, or at least through comparative study of all questionnaires sent by a given person Doing again this work for all 85,000 basic data used in the *Atlas* would be at least as time- and energy-consuming as has been the original work which led to the production of this book Clearly the methodological reflection should have been deepened further *before* starting the work.

Of course, it is clear that no scientific work is free from errors, and one cannot expect a large-scale taxonomic or chorological survey, involving hundreds of collaborators and thousands of data, to be so. However, in order for such a work to deserve the qualification of "scientific", one should expect the rate of errors and omissions to be below a certain level: I have suggested elsewhere (Dubois, 1987*a-c*) that, in this domain like in other scientific fields, an acceptable standard rate of errors and omissions ("EO rate") should be below 5 %. Is this rate respected in the European *Atlas*? For the time being, too little information is available to allow to appreciate *quantitatively* the amount of errors and omissions in this work (except in the case of synonymies examined above, 27 % only of which are "genuine synonymies"). The *Atlas* provides no information on whether, for a given species, the author of the text has seen the original questionnaires or was only provided the final map, whether all the older relevant literature was examined, critically evaluated and computerised, whether data on specimens kept in all major museums were incorporated in the data base, etc. What seems clear is that the way the basic data were obtained and critically studied before computerisation was heterogeneous. Different methodologies were apparently used according to the country, and perhaps also to the taxon studied. While it is nice to see that this work was truly collective and involved several hundred persons, perhaps in a way there were *too many* people involved to obtain a homogeneous high scientific level result. On another hand, despite this high number of collaborators, one is struck by the total absence in the lists of observers and authors of several prominent herpetologists, some of whom have produced significant contributions to European herpetology, such as distribution atlases, field guides, books or scientific papers, and are largely cited in the list of reference at the end of the volume, or of this review: clearly this book was the result of the work of a part only of the community of European herpetologists.

The problems raised above are probably due to two major kinds of causes deficiencies in the methodological reflection before starting the inquiry, and time shortage. This latter problem can be guessed from some statements in the book itself (p. 11: "Because time was pressing"; p. 13: "because time was very short"). It is not unique to this work, rather it is a common problem in current research and scientific publication (see e.g. Dubois, 1987*b*) In particular, this "time shortage" question is often raised for collective books, as publishers do not like to wait undefinitely for completion of the final manuscript and tend to impose precise (and usually close) deadlines to editors and authors. Although this is not justified scientifically, such a hurried attitude is understandable when the publisher is a private company with commercial constraints. Should it be the same when the publishers are a non-profit scientific association (SEH) and state organisms (French Ministry of Environment and SPN)? In such cases, one would expect the major criterion to be scientific quality, not speed of publication (see also Dubois, 1987*b*: 111).

Possibly, for the production of such a volume, the motivations of state organisms are different from those of scientists. In the recent years, state organisms like the French Ministry of Environment have tended to support financially the publication of distribution atlases, checklists or other documents having some connection with conservation problems. In some cases, when one considers the scientific quality of works so produced (often under very short time constraints), one cannot help from wondering whether the primary goal of such publications was scientific accuracy or simply "to have a document", whatever it may be. In several European countries, laws now require that, before undertaking some major works (like building a road, a railway, a dam, etc), a public inquiry be made on the impact that this work is likely to have on the environment and on living species. However, these legislative texts usually only require "to have a study", not that it be scientifically irreproachable or that its conclusions have a binding effect on the conception of the works to be done. In such a context, "having an atlas" might appear as a sufficient goal for such organisms, irrespective of its scientific rigour and quality. Should scientists and naturalists adopt the same goal?

In the recent years, I have talked with many colleagues in different countries and I know that all do not share my attitude on these problems. Some think that it is better to have an imperfect atlas than no atlas at all, or an imperfect checklist than no checklist at all (see DUBOIS, 1987*a-b*). This is in part due to a laudable general positive attitude towards such works, with the idea that the result is "better than nothing", and also to the fact that, as each person of course knows personally well only a part of the data covered by such huge endeavours (be these taxonomic or chorological), it is impossible for any of us to detect all mistakes occurring in such collective works. Quite significantly however, when one talks with people who tend to support such works, in many cases they will tell you that the book is good and reliable, except precisely in the given field (be it taxonomic or chorological) of their particular competence, and in this limited field they will point to mistakes or omissions; often, probably through a nice "act of faith", they will assume that such errors are not as common in the other parts of the book. However, experience shows that exactly the contrary is true: pointing to specific mistakes in the necessarily limited field of one's particular competence (as I have done above) usually allows to disclose the existence of more general methodological problems that will affect all the work

The question that must seriously be asked regarding important collective works such as checklists or atlases is "what is 'better than nothing'?" Is it a seemingly complete work including numerous mistakes, or in incomplete work with a low rate of mistakes? I contend that only the second situation qualifies for the characterization of "better than nothing", while the first one, in some cases, may be "worse than nothing".

The personal responsibility of any researcher when carrying out a scientific work is to make all possible efforts to produce a scientifically irreproachable result, given the material means that have been put at his/her disposal to carry out the work These efforts should bear on all aspects of the work, i.e. carefully defining the research methodology, rigorously applying this methodology to obtain and analyse the results, honestly and competently discussing these results and drawing conclusions, and clearly presenting all these data in a final publication. Although it is clear that a researcher should try his/her best to obtain proper funding and staff support for the research project, he/she cannot be taken responsible for deficiencies in this respect, while he/she can be blamed for bad methodology or insufficiently rigorous work. Science is supported by society as a whole, although of course, in the detail, this financial and human support is provided through various channels, from international to state and to private ones. The support currently given in our societies to scientific research is quite different according to the scientific field at stake, clearly reflecting disparities in the importance that is afforded by our societies to these different research fields. Can one imagine that, a space probe sent to Mars missing the target by a few thousand kilometers, or a dam keeping its water for some years after building and then breaking out, or a HIV-test detecting the presence of the virus in human blood in some cases only, the comments would be: "it was better than nothing"? I am choosing three caricatural examples on purpose. What is common to them is that the aim of the work is considered important for mankind, or at least for some people. On the other hand, why are many zoologists apparently ready to accept that publishing incorrect taxonomic or chorological data is "better than nothing" and should not be criticised? Possibly because, even among zoologists themselves, a poor rating is given to these activities, and to their potential consequences in the real world. What can be the consequences of publishing an incorrect distribution map of *Pelobates fuscus*? These will include an incorrect basic understanding on the history and ecology of the species, i.e. a consequence which "merely" concerns our scientific knowledge of a "negligible" part of nature on our planet, and possibly, as a result, inadapted conservation measures concerning this "obscure" species. Frogs and salamanders are not elephants or whales and, except for a few spectacular ones such as *Mantella*, *Dendrobates* or *Bufo periglenes*, they elicit little interest among non-specialists. Who cared for the virtual extinction of *Triturus alpestris reiseri*? Who will care if *Triturus (alpestris) inexpectatus* becomes extinct? Needless to say, to many people and social groups in our society, such problems are of very weak importance or of no importance at all, so that, for them, an imperfect atlas, rather than "better than nothing", might be regarded as "good enough" for its purpose. Should zoologists share this attitude? If they decide to do so, they should not expect other social groups in our societies to support what should be their own concern. A number of current zoologists whose major activities are in the "traditional" fields of taxonomy, faunistics or inventories seem to be almost "ashamed" of their own work, perhaps because they are impressed by other more recent developments of biology, such as molecular research, phylogenetic analysis or evolutionary ecology (all works which, of course, are of

great theoretical and practical interest, but which deal with other questions). If zoologists do not struggle for these "out-fashioned" activities, who will care for the inventory of biodiversity on our planet before large parts of it are extinct (see DUBOIS, 1997*a*)?

In many respects, the importance of the realisation of such collective works as the European *Atlas* commands admiration, when one considers the efforts produced by many individuals to produce the basic data Most of these observers were amateurs, who had to support personally all the costs implied for them by this inquiry. Is this situation "normal" and desirable? Are space probes sent to Mars, or molecular researches carried out, by enthusiastic amateurs, at their own cost? If the 85,000 basic data of the *Atlas* had to have been collected by competent professional scientists with normal salaries and paid field work expenses, the cost of the inquiry would have been much higher. Of course, according to the current priorities of our societies, such an idea may seem completely irrealistic, if not crazy. Why doesn't it appear irrealistic or crazy to spend incommensurably higher funds to send space probes to Mars? Is it because exploration of space is of much more immediate need and importance for mankind than inventorying, evaluating and conserving biodiversity on our planet? Or is it because the latter aim is regarded of very low priority by most people in charge of taking major decisions in our societies? Questions like this should be seriously considered by those who think that mediocre works should be accepted as "better than nothing", without discussion, in our field of research, or "good enough" for the latter, rather than struggling for much more funds (for research, collections, publications), much more academic and non-academic laboratories, jobs of researchers and technicians, high level courses and diplomae, for the inventory and study of biodiversity. Otherwise, present and future complaints about the impoverishment of this biodiversity, and about the consequences of this fact on the environment, and ultimately on mankind, will be completely hypocritical and inefficient.

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